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ABSTRACT

Noting that in Europe there is a growing criticism of the largely American communication research tradition that focuses on individuals, this paper examines European-style "critical analysis," which looks at social systems. The paper first examines the criticism of American-style "administrative" research, and then discusses two problems of using the social system rather than the individual as the unit of analysis in communication research. Finally, it considers whether the more productive direction for mass communication research is education (the traditional American goal) or generalization (the typical European goal). (FL)

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A Critical Look at Critical Analysis

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A Critical Look at Critical Analysis

In its recent report to Unesco, the International Commission for the Study of Communication Problems (1980) -- the famous MacBride Commission -- made this recommendation for future communication research:

> [R]esearch must aim to draw closer to the emerging consideration of communication as a social process, which entails studying media institutions not in isolation but in their relationship to other institutions in broad social, national and international contexts, i.e., conceptually in terms of structures, ownership, organization, socialization, participation, etc., potentially leading to a reappraisal of existing systems, institutions, structures and means.

Behind this litany of research methods and objectives which, like the rest of the report, acknowledges all points of view, is the growing criticism of the largely American communication research tradition which focuses on individuals. Instead, the report reflects the generally European approach which purports to study social systems rather than individuals.

These differences in the way mass communication is studied on opposite sides of the Atlantic have been well documented of late (Halloran, 1980; Blumler, 1978; Hardt, 1979), perhaps to the point of forcing distinctions without differences (Carter, 1979; Lang, 1979). Nevertheless, within the eclectic craft of communication research, there are

important differences in what we study and how we study it.

Most current criticism is directed against "administrative research" by "critical analysis" researchers who find the former at best too narrowly focused on individual effects and at worst merely a tool of the existing power structure. The MacBride Commission notes:

> Turning its back on the statistical approach, research theory and practice in recent years has (sic) undergone radical changes. This is partly due to critical evaluation of research concepts in developed countries by modern research specialists from these same countries; and partly as a result of experiments carried out in the developing countries. Despite the differences in background and interest, there is now a degree of consensus with regard to the inadequacies of the past and on the need for changes in approaches to communications research.

Maybe. But even if we acknowledge all of the sins of omission and commission of which American-style research stands accused, the alternative is not necessarily an improvement. And, in fact, it may be worse. In this paper, we propose to examine some of the methods by which European-style research proceeds and in particular, whether "critical analysis" research meets the essential test of empirical verification. In short, we propose a critical evaluation of critical analysis.

Our critique will consist of three parts. The first is to examine what is essentially a straw-man criticism of "administrative" research. Then we will look at two problems of using the social system rather than the individual as the unit of analysis. Finally, we will consider whether the more

productive direction for mass communication research is reduction (the traditional American goal) or generalization (the typical European goal).

Conservative Administrative Research?

The MacBride Commission report provides this thumbnail sketch of the development of communication research mostly referring to the United States where, in a footnote it notes, "the resources devoted to research are particularly substantial":

> There have been several stages in the evolution of communications research. Until the thirties, little or none had been done on communications, with the exception of literary criticism, a few studies on propaganda and other surveys commissiond by the media to ascertain their impact upon the public and its tastes. Originally, such studies were designed as isolated projects, focusing on specific problems. The approach was a simple and pragmatic one, geared to mass communication phenomena and the efficacy of this form of communication.
>
> Subsequently, a large number of theoretical and practical models were used in the search for ways of improving advertising, organizing election campaigns, polling public opinion, supporting public relations activities and boosting newspaper circulation. Designed to enable individuals to adapt to the prevailing social norms, and to create media and messages capable of inducing the desired behaviour patterns, these models, inspired by pschyological studies, were to help elucidate the factors motivating individuals considered principally as consumers of information and messages and to determine readers', listeners', and viewers' profiles. Frequently undertaken by the media themselves, such research was designed to supply them with the information they needed for their programming, advertising and market surveys. Research of this kind is chiefly inspired by commercial, electoral or media considerations, and fails to take account of the inter-relationships

between communication and many major problems affecting societies as a whole.

The argument that American research is used primarily to reinforce the existing political and economic system is a common one, and the MacBride Commission's statement is relatively mild. More strident criticism can be found in some of the papers presented at the 1980 IAMCR congress in Caracas (e.g., Capriles, 1980; Hamelink, 1980; Schmucler, 1980; Martelanc, 1980; Szecsko, 1980).

But commercial interests dominate American research only at a superficial level. Certainly most of the money spent on communications research in the United States comes from private interests, but the results of such efforts -- Neilsen ratings, readership and market data, even most public opinion polls -- are intellectually trivial. Outside some developments in sampling and measurement techniques, it is hard to find anything commercial research has contributed to the fragments of communiction theory or research methods.

In fact, the handful of fragments of theory that exist is almost entirely the product of non-commercial research. Four major research projects -- by no means the only ones that have had a lasting influence -- illustrate the point. The Yale studies of social influence (Janis, et al., 1959), which really started a major branch of American communoication research, were, of course, funded by the U. S. Army as part of its training in World War II. The Columbia University studies from

which the two-step flow hypothesis was derived represent the other pillar of the foundation of American research. The first study, which was reported in the influential book The People's Choice (Lazarsfeld, et al., 1948), was a voter survey of the 1940 presidential election in small town Ohio. The second study, reported in Personal Influence (Katz and Lazarsfeld, 1955), was supported by Macfadden Publications, but it is hard to find any items in the questionnaire that would have direct market value to that organization.

A study which is often cited as marking the point at which communication research split off from the earlier studies of social influence and persuasion was the study of community relations commissioned by the Palo Alto School Board. From it came the Carter article (1965) which may well be the most-cited reference in recent research. A more recent example of highly influential and productive middle-range theory fragments is the agenda-setting hypothesis. The first agenda-setting studies were carried out without any outside funding, and the first (and to date, only) book-length research report (Shaw and McCombs, 1977) was carried out with a modest academic grant.

What all of these studies have in common is a solid lack of connection with any commercial sponsorship or purpose. They also share a concerted and successful effort to build and test theory. It is the latter quality which makes them memorable. It would be hard to find very many people who remember whether the film "The Battle for Britain" was an effective vehicle for

troop indoctrination or the kind of community attitudes that existed toward the Palo Alto School Board. But the importance of source credibility and message structure and the distinction between salience and pertinence are remembered and for good reason.

Too much of our research -- commercial or otherwise -- lacks any theoretical basis, and for that we stand accountable to ourselves as well as colleagues on the other side of the Atlantic. Tipton (1978), summarizing a decade or two of readership studies for the American Newspaper Publishers Association, concluded that "almost any conceptual focus yields better statistical predictors of newspaper use than do isolated demographics" which have dominated most industry and academic research.

Neither newspapers with an obvious commercial interest in maintaining readership nor academics with both a theoretical and civic interest in understanding mass media have got very far along usually separate but parallel paths. But if our impressive ability to collect and analyze data is overshadowed by an inability to generate theory to guide data collection and organize findings, the European school is at least as weak in generating broad-based social theories which stand the test of empirical verification. We shall apply the test of empirical verification to two examples of European research and, in doing so, point out problems of seeking understanding of human behavior at the social system rather than individual level.

Individuals vs. Social Systems in Research

An essential component in much of what (for a lack of a better term) we are calling the European tradition of the study of mass communication is to emphasize the society itself, rather than the individuals in the society, as the unit of analysis. This is within the tradition of the social sciences which are defined by the level at which they seek explanation: anthropology studies cultures, sociology studies groups, psychology individuals, and so on. In each case, explanation is sought in terms of the smaller unit: sociology seeks to explain the behavior of cultures in terms of the behavior of groups, psychology tries to explain the behavior of groups in terms of individuals, and so on. The process of reduction moves through the social sciences to the biological sciences and on to the natural sciences. The goal of all science is to derive the ultimate building blocks of the universe and the principles by which it operates. With these components, it will be possible -- at least theoretically, at some far-distant point in time -- to "reconstruct" the universe, moving this time from the natural sciences and biology to the social sciences.

The power of this essentially Western approach to knowledge, on one hand, is awesome. It has led to the exploration of the universe in the 1960s and 1970s and truly revolutionized our understanding of the cosmos. The explosion of scientific and technical knowledge resulting from this odyssey of space exploration is surely responsible for the

dominant position of the West as the source of most of the new information in the world. And the decades of the 1980s and 1990s promise to produce an equivalent revolution in the biological sciences. The promise of these upcoming decades is to unlock the mysteries of living species -- for good or evil -- in a way that will make the advances in knowledge of our day seem modest.

But on the other hand, the primitive efforts of social science to fit humankind into this grand system have been, one must admit, unimpressive. Social science is, figuratively speaking, in the 1600s, uncertain whether the earth revolves about the sun or the sun about the earth. We do not know the basic unit of human behavior and have only the smallest fragments of theory, imperfectly defined and poorly tested, with which to reconstruct the universe of social science.

Of course, we need not be too harsh on ourselves. At the level of human behavior, we have to deal with factors like consciousness, free will, intention and inventiveness. Our natural science colleagues merely have to understand the universe; we have to understand ourselves.

What is wrong with looking at societies rather than people, of seeking explanations of human behavior in the social system rather than in the individual? Two aspects of the way most critical analysis research is carried out, we argue, tend to invalidate results.

Ecological fallacy. Halloran (1978), one of the best

international scholars and one who, better than anyone else, bridges the gaps among research traditions, criticized the Surgeon General's report on the effects of televised violence for asking the wrong question. While not absolving television from responsibility, he argues (and cites the 1969 report of the National Commission on the Causes and Prevention of Violence for support) that social and economic inequities are a more significant factor and that violence in some cases is a mechanism for maintaining social control.

He also distinguishes among different kinds of violence:

> Most people think of violence, both in the collective and individual sense, in terms of assassinations, murders, riots, demonstrations, assaults, robberies, rapes, acts of vandalism, and so on. In fact, for many, this sort of "illegitimate" behavior represents the totality of violence. But there are others who adopt a different and wider approach and include in their definition war, capital punishment, corporal punishment and certain aspects of penal practice, police behavior and school discipline. A still broader definition might include poverty, deprivation, economic exploitation and discrimination -- and it has been argued that the behaviors in these categories are interrelated and feed off each other. In fact, society may contribute to violence by the seal of approval it gives to certain forms of violence, particularly those that have been legitimized in the name of social control.
>
> There is no need here to favor any one of these definitions, and it is accepted that valid distinctions can and must be made between different kinds of violence.

Later, citing the Kerner Commission's conclusion that "probably the most important cause of major increases in group violence is the widespread frustration of socially deprived

expectations about the goods and conditions of life men believe theirs by right," he comments: "The above passages from the report to the National Commission are couched mainly in terms of group or collective protest or political behavior, but they are applicable mutatis mutandis to individual behavior as well."

It is this shifting from one unit of analysis to another that makes so much of critical analysis difficult to understand. And the movement from one level to another, mutatis mutandis, is unsound. Measurement at one level of observation and inference about behavior at another level is the ecological fallacy (Robinson, 1950; Langbein and Lichtman, 1978). The problem of ecological fallacy is a confusion of the forest and the trees or more accurately, observing one and drawing inferences about the other. The problem usually arises because researchers want to study individuals but only have data at the aggregate level -- census tracts, political units, etc.

The opposite problem -- measuring individuals but making inferences about groups -- also arises on both sides of the Atlantic. American researchers have run into it when they try to use averages of individual measurements to represent some undefined collective measure. Chaffee (1972) referred to "pseudo-data" in raising the question of whether collectivities like "the public" really exist and whether averages of individual measures can be anything more than reifications. The difficulty has been encountered in several major American research efforts -- coorientation (where individuals often are

asked to estimate a collective measure that may not exist), agenda-setting (where averages of individual measures are assumed to represent collective expressions) and, of course, public opinion research where the existence of "the public" distinctive from an aggregate of individuals is assumed but not demonstrated.

Critical analysis researchers frequently fall victim to it because they are often unclear whether they are measuring individuals as individuals, measuring individuals and inferring characteristics of social systems, measuring social systems and making inferences about individuals within those systems or linking some other combination of individual and social measurement. It is not hard to find examples of this problem.

To demonstrate the ecological fallacy, we can look at serious crime as one instance of social violence and relate it to economic conditions -- unemployment, poverty and per capital income -- that both Halloran and the Kerner Commission emphasized as major sources of violence.

For our analysis, we will use data from federal government statistics aggregated at the state level. The unit of analysis is the state (N=51); the variables are aggregated at that level. Data are from 1977-78. The variables are the number of "serious crimes" (a category that includes mostly violent offenses but also larceny and auto theft) per 100,000 population, percent unemployment, percent below the federal poverty level and per capita income.

In Table 1, we see the correlations and coefficients of determination between the level of violent crime and the three economic criteria, all measured in terms of state averages. All of the correlations are substantial by conventional social science standards, ranging from .34 to .56. Statistical significance is not at issue because we are dealing with the population of states.

Table 1 about here

The best correlate is per capita income: crime rises along with income (r= +.56). But crime also rises with unemployment (r= +.34) but falls as the percentage of the population living in poverty increases (r= -.35). This at first appears to be a curious outcome. Do we conclude that violent crime is committed by relatively wealthy, unemployed people, while the working poor are law abiding?

Of course not. At the time the data were collected, violent crime was -- and maybe still is, although some evidence suggests that the pattern is changing -- especially prevalent in the industrial North where per capita income and unemployment were relatively high and poverty relatively low. In contrast, in the sunbelt, poverty was relatively widespread while unemployment and income were relatively low.

What can we conclude from this analysis, either about crime at the individual or the social level? Nothing really, because

our data are at the aggregate level while our real interest is the behavior of individuals. It is possible that some characteristic of the state would be of interest -- for example, differences in judicial or penal system, the presence or absence of the death penalty, etc. But it is not clear that these kinds of differences are related to crime rates at the aggregate level or, more important, that the threat (or lack of threat) of punishment has much influence on individuals.

Langbein and Lightman say ecological inference is possible if certain assumptions are met, but the irony of their argument is that they assume we want individual measures but cannot obtain them; they do not consider the possibility that we might have access to individuals but would want to abstract (or reify) the individual measures to represent some collective value.

Generalizing from a sample of one. Another problem critical analysis frequently faces is generalizing from studies of single social systems -- case studies, really -- to the behavior of systems as a whole. A good example is a recent study of the effects of commercial broadcasting on national development in the Third World. The issue is of considerable interest because the MacBride Commission recommended an emphasis on non-commercial broadcasting in the Third World, to the vocal and sometimes unreasoned opposition of American commercial broadcasters.

Paul Hartmann, a colleague of Halloran at Leicester and a skilled and thoughtful researcher whose integration of

quantitative and qualitative methods deserves our emulation, reported in 1978 a study in the Philippines. It evaluated the work of the Communication Foundation for Asia, a non-profit organization which produces a range of development-related materials, among them radio dramas. Hartmann and colleagues interviewed a sample of Filipinos about media use, uses and gratifications derived from mass media and attitudes toward local and national problems. He reported this finding in a preliminary paper (Hartmann, 1978):

> In many cases there was a sense of helplessness or an attitude of fatalistic resignation in the face of the severe hardships that beset these communities; or people looked to the government or other authorities for solutions. At the same time, and sometimes co-existing with this type of attitude, the view was repeatedly expressed that local initiatives or communal action by the people themselves could go some way towards improving things.

Hartmann also analyzed a sample of popular soap-opera-like dramas on commercial radio and another sample produced by CFA. In the former, he found values which supported both fatalism and consumerism, while the latter stressed individual and community initiative and "producerism."

Without presenting any evidence of having analyzed the data at the individual level -- that is, correlating producerism vs. consumerism, fatalism vs. initiative with frequency of exposure with the two types of radio dramas -- Hartmann concluded that the commercial radio dramas were instrumental in the development of attitudes dysfunctional to development. He also noted that

the pro-development CFA dramas could attract an audience and, in fact, were the fourth-rated radio program in Manila, but:

> Taking the survey findings in conjunction with the content (of the commercial dramas), it is clear that the main things that people were getting out of their drama listening were recipes for interpersonal adjustment, a degree of value reassurance, and some diversion that presumably helped to make life more bearable. It does not follow from this that the bulk of the drama was therefore simply irrelevant to development. On the contrary, there is every reason to think that much of it was counterdevelopmental in character. For one thing, a great deal of the material had strong "consumerist" overtones through its portrayal of well-do-do urban life-styles. But a probably more important point is that the broader message implicit in so much of the drama was one of passive resignation.

Hartmann concludes that commercial broadcasting is dysfunctional to economic and social development because it cannot or will not produce programs supporting development and because consumer-oriented advertising is itself dysfunctional to development:

> You do not have to be a crude economic determinist to recognize that when the economic underpinning of a system of cultural production is closely intermeshed with the fortunes of the urban commercial sector of a poor country -- as is the case with radio in the Philippines -- all the pressures are in the direction of the reproduction of consumerist rather than a producerist values, and that this is likely to occur even in spite of contrary intentions on the part of those involved in the process of production itself. You also do not have to import cultural products themselves -- music, films, television programs, for instance -- to generate or sustain cultural valaues that maybe harmful to development; it is quite possible to import only a particular system of production and market organization in order to produce equally harmful home-grown products.

While not arguing that these results necessarily can be generalized to other countries, Hartmann clearly is interested in more than the Philippines as a unique case study. What can we say about his method?

First, we can ask whether the conclusions can be verified empirically. As always when the unit of analysis is the social system, we run into problems of finding adequate, reliable data. While we would all like to replicate our studies in as varied social settings as possible and seldom can, it is imperative that we get some variance in the independent variable when the unit of analysis is the social system and the independent variable is some attribute of political or economic structure.

In this case, we can approximate a replication of Hartmann's analysis by using data from the United Nations and the World Bank. The population in this case consists of the 140 or so member states of the UN so we need not worry about sampling. The only problem is the lack of observations on some cases and the appropriateness of the data that are available. In this case, we can use Unesco figures which characterize broadcast systems as commercial, public, government or a combination. And from the World Bank, we can get figures of per capita GNP, not exactly a measure of attitudes toward development but certainly one measure of the level of economic development.

Table 2 about here

Table 2 is based on only the 113 Third World countries for which we have data on both media ownership and per capita income. It is striking that 75 out of the 113 have either a government or public system; only 9 have exclusively a commercial system. We could argue that the problems Hartmann warns of are pertinent in only a small part of the Third World, although, of course, the definitions get muddy. Many government and public systems do carry some commercials, and some are operated like the entertainment-oriented commercial system.

To get as close to the Hartmann argument as we could, we put all systems which had at least some element of commercial radio (38) into one group and all those with only public or government systems (75) into another.

Table 2 shows the average per capita income in each group. A t-test is not appropriate because we are approximating the entire population of Third World countries. Is there evidence here that commercial broadcasting in all or part promotes or hinders economic development?

No real analysis is possible. For one thing, the variances in each group are so large that any comparison is meaningless. And, of course, per capita income within any one country or among countries is a poor approximation of the "producerist" attitude Hartmann was discussing. But we can address, in a very general way, the basic issue of the relationship between the ownership of a broadcasting system and the level of economic

development. And if we cannot argue that commercial broadcasting is a positive factor in development, we are on equally shaky ground to argue the opposite. In fact, we have no evidence in these data to support Hartmann or to challenge him.

Even if we had more appropriate data -- some measure of public attitude across a range of countries with differing media systems, for example -- could we link media ownership to social effects? Probably not. The most obvious objection is that such an analysis overlooks a whole range of pertinent variables -- control, content, availability, audiences and, of course, effects, which vary independently of ownership. By concentrating exclusively on ownership, neo-Marxist analysts (Schiller, 1969), who would find the Hartmann conclusions compatible with theirs, regress to a simplistic version of stimulus-response: ownership --> everything. As we have seen here, such an analysis is neither consistent with what data we do have that approximate social-level measures nor an adequate basis for dealing with the broad range of variables that critical analysts (and the MacBride Commission) argue is necessary to understand mass communication in its social setting.

Two of the problems we encounter here are the same as we noted above: the absence of empirical support for the conclusions drawn from a critical analysis of a single social system and the confusion of individual and collective attributes and measurements. But another problem arises as well: it is

the question of what kind of generalizations -- if any -- can be drawn from samples of one.

Hartmann collected a lot of diverse data, but by using them to describe a single social system rather than individuals, he really produces a single data point in a paradigm that is outlined in Figure 1. The independent variable is Broadcast System, categorized as Commercial and Other; the dependent variable is Public Attitude Toward Development, categorized as Functional and Dysfunctional.

Figure 1 about here

We might want to make some refinements in this paradigm, but the arguments of critical analysis in general can be arrayed in this fashion: the independent variable is the political or economic system or some attribute of it like media system, and the dependent variable is some effect on individuals, usually measured at the aggregate level.

With a single data point, no inference at all can be made because concomitant variation is not demonstrated. Without evidence of systematic variation, the issues of time order and alternative explanations cannot be considered.

Where would a second data point fall? The necessary corollary of Hartmann's conclusion is that public attitudes would be functional to national development in a system which did not have commercial broadcasting, that is, that a second

data point would fall in the upper right cell. Problems with development are similar in many Third World societies, and commercial broadcasting is the exception rather than the rule in the Third World. Because of these factors and our examination of per capita GNP by broadcast system which does not support that conclusion, another data point from a country similar to the Philippines but with non-commercial broadcasting is likely to fall in the lower right cell. If so, the explanation of commercial broadcasting as the reason for the dysfunctional attitudes in the Philippines becomes more suspect.

With one data point, any explanation is plausible. And suspect. Or more generally: Any explanation will account for any single observation. Without evidence of concomitant variation --without some comparison with a different system -- there is no basis for assuming that the explanation proffered is any better than any other.

This, of course, is a particular problem with Marxist analysis. It can be used to provide a superficial and incomplete explanation of capitalist societies or individual behavior within those societies. But what Marxism does not explain very well is socialism. Of course, there are special difficulties doing research in those countries, but it would strengthen the arguments of those who try to explain the behavior of individuals in capitalist societies on the basic of the economic structure if they could demonstrate that things were any different in other systems. It is hard to find a

theory based on the structure of the social system since Marxism that has much empirical support and none with evidence of concomitant variation across a range of social systems.

In fact, if one looks at the behvior of individuals in various countries, what is impressive is the similarity of human behavior, not the difference. For example, there seems to be some kind of Gresham's Law in mass media that bad programming drives out good. It is certainly true that the mass-appeal, lowest-common-denominator programming so popular on American TV has universal appeal. It may lack any redeeming social value and even be dysfunctional to economic and social development, but it attracts viewers in the Philippines or Poland or Peru for the same reason that it gets 30 percent audience shares in the United States.

We occasionally get data about program preferences in societies like the Soviet Union (Hollander, 1972) where the medium serves very different purposes. What is popular there? Sports and light entertainment with political programming at the bottom of the list. The ranking reads like a Nielsen rating report.

Even at the level of news media content -- much closer to social behavior -- the similarities can be surprising. We do not suggest that the New York Times is really the same as Pravda, but the overwhelming conclusion of the IAMCR-sponsored study of foreign news (Stevenson, Cole and Shaw, 1980) is the nearly universal nature of foreign news. In everyone of the 29

countries in that study -- covering, as Unesco requested, societies at all stages of development and with all types of political systems -- foreign news emphasized the immediate geographic region and concerned mostly politics and political leaders. News is a timely account of exceptional events, mostly in the region. And this is pretty much the case in the media of countries from Argentina to Zambia. If we were to present the results of this study -- and 29 data points is a large number for a project like this -- the analysis would look something like Figure 2. No evidence of concomitant variation here.

Figure 2 about here

The critical analysis arguments about the nature of news in the West can be used to explain why the news is narrowly defined, regionally centered, good or bad in those countries of the First World. And in some cases, it can be argued that the influence of the West in the Third World -- whether a result of neo-colonialism, or otherwise -- can also explain why some of the Third World countries have a similar foreign news content. But what about the Soviet Union and the countries of Eastern Europe? And for that matter, Third World countries like Zambia, Tunisia and Algeria which have undergone some kind of political-social revolution which included "liberation" of their news media? How can we explain the lack of difference in foreign news coverage?

What is offered in documents like the MacBride Commission report or the IAMCR papers as an alternative to narrowly focused "administrative research" is something of a chimera. What purports to be measures of social behavior is often pseudo data, reifications of individual measurements presented as the behavior of non-existent collectivities. And "critical analysis" of the social influences on individuals is often an elaborately constructed explanation presented as a generalization applicable across societies. In fact, it seldom meets any of the criteria for causal inference, concomitant variation, time order and, most important, elimination of other, equally tenable, explanations.

One can accept most of the criticism of the research tradition which seeks explanation of human behavior in the individual and yet find even less in the alternative approach. Is the future of communication research doomed to repeat the medieval arguments about what is at the center of the solar system and what revolves around what?

We can take some solace in remembering that it took 3,000 to 4,000 years of observation before Newton discovered (or invented) the equations that explained a mathematical universe. Or that 300 years elapsed before Einstein provided a higher order explanation. As researchers concerned with human behavior, our subject is infinitely more complex, and we have not been at the task very long.

But one of the purposes of gatherings like this is to

consider which directions we ought to go. And we recommend a strategy that adheres to scientific reduction with insistence on empirical verification, multiple operationalization and testing of necessary consequences of theoretical propositions. The alternative approach we have criticized is likely to degenerate to polemic rather than to build knowledge. And with the natural sciences as models and guides, we have the power of lower-order sciences to serve as a firm foundation for a true science of human behavior.

The Direction of Future Research

This new direction, of course, is not new; it is really a restatement of Descarte's four rules of logic (Bronowski and Mazlish, 1960):

> My first rule was to accept nothing as true which I did not clearly recognize to be so; to accept nothing more than what was presented to my mind so clearly and distinctly that I could have no occasion to doubt it.
>
> My second rule was to divide each problem or difficulty into as many parts as possible.
>
> The third rule was to commence my reflections with objects which were the simplest and easiest to understand, and rise thence, little by little, to knowledge of the most complex.
>
> The fourth rule was to make enumerations so complete, and reviews so general, that I should be certain to have omitted nothing.

Recent developments in biology give us a glimpse of what the next few decades may produce by way of understanding of the human brain as a physiological organ. Already some mental

functions can be located in the brain and defined chemically. Beyond that, scientists may be on the verge of understanding how sensory stimuli -- the basic input of human communication -- are processed by the brain as electro-chemical energy. Studies of this very basic behavior, both research on the brain itself and computer-based research on artificial intelligence, are exciting and provocative.

But this is a long way from the study of human social behavior. The gap between people as biological entities and people as social, thinking animals, of course, is a huge chasm. Some halting efforts to bridge that gap are underway, and we need not wait for the full flowering of biology to profit from its knowledge. Some tentative and admittedly speculative efforts to link social and biological behavior can be found in Sagan's provoctive The Dragons of Eden (1977) and Wilson's pioneering efforts (1978) to define a new field of sociobiology.

Beyond that, there is a definite trend among a number of North American reserchers to emphasize basic research. This involves a recognition that communication is a behavior which includes paying attention to (or ignoring or avoiding) information, processing information, using information to construct ideas which become the basis of human actions. These surely are the building blocks for the study of communication as human behavior. At recent meetings of this organizations and companion groups, this trend was evident in papers which emphasized how people -- particularly children -- process

information they receive from the mass media. These are laudable efforts to get inside the black box of the mind which in prior research has been considered outside the range of empirical verification.

A comparable emphasis in identifying a basic unit of cognitive behavior -- a kind of behavioral quark -- and sequences of behavior that constitute familiar concepts like opinion formation and change is evident in Edelstein's reconceptualization of public opinion (1974) and Carter et al's (1973) Signaled Stopping Technique which provides a theoretical basis for defining cognitive units and a trace of them.

All of these efforts point toward a recognition of the individual as the center of any social science and individual behavior as the building block for any explanation of the complex workings of human affairs. To examine the influence of the superstructure of social, political and economic institutions, we can consider individuals as nested within different systems and develop analytical procedures accordingly. But this is very different from computing aggregates of individual measures and calling them social measures or analyzing characteristics of political states when we are interests are in the behavior of individuals within those states.

What we need at this point is a willingness to profit from the criticism directed at both schools of research. We can recognize that data without adequate theory is intellectually

sterile, but we also must acknowledge that theory without data is merely polemic. As we grope toward understanding of the human species, the natural sciences can be both a starting point and a guide. Wilson offers a glimpse of both the path and the destination:

> Reduction is the traditional instrument of scientific analysis, but it is feared and resented. If human behavior can be reduced and determined to any considerable degree by the laws of biology, then mankind might appear to be less than unique and to that extent dehumanized. Few social scientists and scholars in the humanities are prepared to enter such a conspiracy, let alone surrender any of their territory. For this perception, which equates the method of reduction with the philosophy of diminution, is entirely in error. The laws of a subject are necessary to the discipline above it, they challenge and force a mentally more efficient restructuring, but they are not sufficient for the purposes of the discipline. Biology is the key to human nature, and social scientists cannot afford to ignore its rapidly tightening principles. But the social sciences are potentially far richer in content. Eventually they will absorb the relevant ideas of biology and go on to beggar them. The proper study of man is, for reasons that now transcent anthropocentrism, man.

Table 1: Correlation of serious crime and economic conditions by state.

		N=51
Unemployment	r=	+.34
Percent below poverty		-.35
Per capita income		+.56

Table 2: Average per capita income by broadcasting system in the Third World.

Broadcasting system	N	Mean	Standard deviation
Non-commercial only	75	$927	$2215
Some commercial component	38	$1773	$4012

Figure 1: Generalization from critical analysis case study

Public attitude toward development:	Broadcasting system: Commercial	Other
Functional		
Dysfunctional	1	

r=?

Figure 2: Characteristics of foreign news in different parts of the world (IAMCR study)

Foreign news:	Geography: West	Socialist countries	Third World
Good, diverse, etc.	0	0	0
Bad, narrow, etc.	8	4	17

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